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ESSAYS

CHEMICAL, ELECTRICAL, & GALVANIC.

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ESSAYS,

&c. &c.

ESSAY,

ON THE QUESTION, WHETHER HEAT CAN BE ASCRIBED TO
MOTION.

IN two Memoirs, published in Professor Silliman's Journal of Science and the Arts, I have endeavoured to show, that caloric and the electric fluid are collateral agents in galvanism—the ratio of the former to the latter, in quantity, being as the extent of the operating superficies, to the number of pairs into which it may be divided.

It was of course assumed, in those publications, that the causes of heat and electricity, are material. Although this view of the source of calorific repulsion, is taken by a great majority of chemists, it has been combated, both by Rumford, and Davy : the former, famous for his ingenious, instructive, and laborious experiments—and the latter, distinguished by the most splendid discoveries. With the utmost deference for the authority of these great men—especially the latter—I offer the following remarks, made in answer to his hypothetical views, which I shall here quote from his Elements, in order to introduce the subject more intelligibly.

“It seems possible,” says the illustrious author, “to account for all the phenomena of heat, if it be supposed, that in solids the particles are in a constant state of vibratory motion, the particles of the hottest bodies moving with the greatest velocity, and through the greatest space—that, in fluids, and elastic fluids, besides the vibratory motion, which must be conceived greatest in the last, the particles have a motion round their own axes, with different velocities, the particles of elastic fluids moving with the greatest quickness—and that, in ethereal substances, the particles move round their own axes, and separate from each other, penetrating in right lines through space. Temperature may be conceived to depend upon the velocities of the vibrations; increase of capacity on the motion, being performed in greater space; and the diminution of temperature, during the conversion of solids into fluids or gases, may be explained on the idea of the loss of vibratory motion, in consequence of the revolution of particles round their axes, at the moment when the body becomes liquid or æriform—or from the loss of rapidity of vibration, in consequence of the motion of the particles through greater space.

“If a specific fluid of heat be admitted, it must be supposed liable to most of the affections which the particles of common matter are assumed to possess, to account for the phenomena; such as losing its motion when combining with bodies—producing motion, when transmitted from one body to another—and gaining projectile motion, when passing into free space; so that many hypotheses must be adopted to account for its mode of agency, which renders this view of the subject less simple than the other. Very delicate experiments have been made, which show that bodies, when heated, do not increase in weight. This, as far as it goes, is an evidence against a specific subtile elastic fluid, producing the calorific expansion; but it cannot be considered as decisive, on account of the imperfection of our instruments. A cubical inch of inflammable air, requires a good balance to ascertain that it has any sensible

weight—and a substance bearing the same relation to this, that this bears to platinum, could not perhaps be weighed by any method in our possession.”

These suggestions of Sir H. Davy, are, to me, unsatisfactory.

It is fully established in Mechanics, that when a body in motion is blended with, and thus made to communicate motion to, another body, previously at rest, or moving slower, the velocity of the compound mass, after the impact, will be found, by multiplying the weight of each body, by its velocity, and dividing the sum of the products, by the aggregate weight of both bodies. Of course it will be more than a mean, or less than a mean, accordingly as the quicker body was lighter or heavier than the other.

Now, according to Sir Humphry Davy, the particles of substances which are unequally heated, are moving with unequal degrees of velocity. Of course, when they are reduced by contact to a common temperature, the heat, or what is the same, (in his view) the velocity of the movements of their particles, ought to be found, by multiplying the heat of each by its weight, and dividing the sum of the product by the aggregate weight. Hence if equal weights of matter be mixed, the temperature ought to be a mean—and if equal bulks, it ought to be as much nearer the previous temperature of the heavier substance, as the weight of the latter is greater; but the opposite is, in most instances, true. When equiponderant quantities of mercury and water are mixed, at different temperatures, the result is such as might be expected, were the water twenty-eight times heavier; so much nearer to the previous temperature of the water, is that of the mixture.

It may be said, that this motion is not measurable upon mechanical principles. How, then, I ask, does it produce mechanical effects? These must be produced by the force of the vibrations, which are, by the hypothesis, mechanical: for whatever laws hold good, in relation to moving matter in mass, must operate in regard to each particle of that matter: the effect of the former, can only be a multiple of that of the

latter. Indeed, one of Sir Humphry Davy's reasons, for attributing heat to corpuscular vibration, is, that mechanical attrition generates it. Surely, then, a motion, produced by mechanical means, and which produces mechanical effects, may be estimated on mechanical principles.

In the case cited above, the power of reciprocal communication of heat in two fluids, is shown to be inconsistent with the views of this ingenious theorist. If we compare the same power in solids, the result will be equally objectionable.—Thus, the heating power of glass being 443, that of an equal bulk of lead will be 487, though so many times heavier; and if equal weights be compared, the effect of the glass, will be four times greater than that of the lead.

If it be said, that the movements of the denser matter are made in less space, and therefore require less motion—I answer, that if they be made with equal velocity, they must go through equal space in the same time, their alternations being more frequent. And if they be not made with the same velocity, they could not communicate to matter of a lighter kind, a heat equally great—since, agreeably to experience, no superiority of weight will enable a body, acting directly on another, to produce in it a motion quicker than its own.

Consistently with this doctrine, the particles of an æriform fluid, when they oppose a mechanical resistance, do it by aid of a certain movement, which causes them effectively to occupy a greater space than when at rest. It is true, a body, by moving backwards and forwards, may keep off other bodies from the space in which it moves. Thus, let a weight be partially counterbalanced, by means of a scale beam, so that, if left to itself, it would descend gently. Place exactly under it, another equally solid mass, on which the weight would fall, if unobstructed. If, between the two bodies, thus situated, a third be caused to undergo an alternate motion, it may keep the upper weight from descending, provided the force with which the latter descends, be no greater than that of the movement in the interposed mass, and the latter acts with such celerity, that, between each stroke, the time be too

small for the weight to move any sensible distance. Here, then, we have a case analogous to that supposed—in which, the alternate movements or vibrations of matter, enable it to preserve to itself a greater space, in opposition to a force impressed; and it must be evident, that lengthening or shortening the extent of the vibrations of the interposed body, provided they are made in the same time, will increase or diminish the space apparently occupied by it, as the volume of substances is affected by an increase or reduction of heat. It ought, however, to be recollected, that, in the case we have imagined, there is a constant expenditure of momentum, to compensate for that generated in the weight by gravity, during each vibration. In the vibrations conceived to constitute heat, there is no generating power to make up for this loss. A body preserves the expansion, communicated by heat in vacuo, where, insulated from all other matter, the only momentum, by which the vibrations of its particles can be supported, must have been received before its being thus situated. If we pour mercury into a glass tube shaped like a shepherd's crook, the hook being downwards, and the orifice closed, the included air will prevent the mercury from occupying a portion of the tube. In this case, according to the hypothesis in question, the entrance of the mercury into the space which the air occupies, is resisted by a series of impalpable gyratory movements; so that the collision of the aerial particles against each other, causes each to occupy a larger share of space, in the manner above illustrated, by the descending weight and interposed body. The analogy will be greater, if we suppose a row of interposed bodies, alternately striking against each other, and the descending weight; or we may imagine a vibration in all the particles of the interposed mass, equal, in aggregate extent and force, to that of the whole, when performing a common movement. If the aggregate force of the vibrations made by the particles, be equivalent to that which, when performed in mass, would be necessary to preserve a certain space—it may be supposed productive of a substance like the air, by which the mercury

is resisted. But, in such rare media, whence can momentum be derived, adequate to resist the pressure of a fluid so heavy as mercury, which, in this case, performs a part similar to that of the weight, cited for the purpose of illustration?—If it be said, that, the mercury and glass being at the same temperature as the air, the particles of these substances vibrate in a manner to sustain the aërial pulsations; I ask, when the experiment is tried in an exhausted receiver, how is momentum to be supplied to the mercury and glass? There is no small difficulty in imagining, that, under the most favourable circumstances, a species of motion, existing, according to the hypothesis, as the cause of expansion in a heated solid, should cause a motion productive of fluidity or vaporization—as when, by means of a hot iron, we convert ice into water, and water into vapour.

Is it not inconceivable, that the iron boiler of a steam engine should impart, to the particles of water, a motion, so different from any which it can itself possess, and, at the same time, capable of such wonderful effects, as are produced by the agency of steam? In particles, whose weight does not exceed a few ounces, can sufficient momentum be accumulated, to move as many tons?

There appears to me another very serious obstacle to this explanation of the nature of heat. How are we to account for its radiation in vacuo, which the distinguished advocate of the hypothesis has himself shown to ensue? There can be no motion without matter. To surmount this difficulty, recourse is had to a suggestion of Newton, that the calorific vibrations of matter may send off radiant particles, which lose their own momentum in communicating vibrations to bodies remote from those, whence they emanate. Thus, according to Sir Humphry, there is radiant matter producing heat, and radiant matter producing light. Now, the only serious objection made by him, to the doctrine which considers heat as material, will apply equally against the existence of material calorific emanations. That the cannon, heated by friction in the noted experiment of Rumford, would have ra-

diated as well, as if heated in any other way, there can, I think, be no doubt; and as well in vacuo, as the heat excited by Sir Humphry in a similar situation. That its emission in this way, would have been as inexhaustible as by the conducting process, cannot be questioned. Why, then, is it not as easy, to have an inexhaustible supply of heat, as a material substance, as to have an inexhaustible supply of radiant matter, communicating the vibrations, in which he represents heat to consist?

We see the same matter, at different times, rendered self-attractive, or self-repellent; now cohering in the solid form with great tenacity—and now flying apart with explosive violence, in the state of vapour. Hence the existence, in nature, of two opposite kinds of reaction, between particles, is self-evident. There can be no property without matter, in which it may be inherent. Nothing, can have no property. The question then is, whether these opposite properties can belong to the same particles. Is it not evident, that the same particles cannot, at the same time, be self-repellent, and self-attractive? Suppose them to be so—one of the two properties must predominate; and in that case, we should not perceive the existence of the other. It would be useless, and the particles would, in effect, possess the predominant property alone, whether attraction or repulsion. If the properties were equal in power, they would annihilate each other, and the matter would be, as if void of either property. There must, therefore, be a matter, in which the self-repellent power resides, as well as matter in which attraction resides.

There must, also, be as many kinds of matter, as there are kinds of repulsion—of which, the affinities, means of production, or laws of communication, are different. Hence I do firmly believe in the existence of material fluids, severally producing the phenomena of heat, light, and electricity.—Substances, endowed with attraction, make themselves known to us, by that species of this power, which we call gravitation, by which they are drawn towards the earth, and are

therefore heavy and ponderable ; by their resistance to our bodies, producing the sensation of feeling, or touch—and by the vibrations or movements which they excite in other matter, affecting the ear with sounds, and the eye by a modified reflection of light. Where we perceive none of these usual concomitants of matter, we are prone to infer its absence.—Hence ignorant people have no idea of air, except in the state of wind ; and, when even in a quiescent state, designate it by this word. But that particles, endowed with self-repellent power, should not be thus perceived, is far from being a reason for doubting their existence. A very slight attention to their qualities, will make it evident, that they could not produce any of the effects, by which the existence of matter, in its ordinary form, is recognized. Repelling each other, they cannot resist penetration : possessing no sensible weight, the greatest velocity can endow them with no perceptible momentum. When in combination, *they* are not perceived, but the *bodies* with which they combine ; and it is only by the changes thus produced, or their influence upon our nerves, that it were reasonable to expect any indication of their existence.

ESSAY

ON THE GALES EXPERIENCED IN THE ATLANTIC STATES OF NORTH AMERICA.*

OF the gales experienced in the Atlantic States of North America, those from the north-east, and north-west, are by far the most influential : the one, remarkable for its dryness—the other, for its humidity. During a north-western gale, the sky, unless at its commencement, is always peculiarly clear, and not only water, but ice, evaporates rapidly. A north-east wind, when it approaches to the nature of a durable gale, is always accompanied by clouds, and usually by rain or snow. The object of the following essay, is to account for this striking diversity of character.

When, by a rise of temperature, the lower portions of a non-elastic fluid are rendered lighter, than those which are above them, an exchange of position must ensue. The particles which were coldest at first, after their descent, becoming the warmest, resume their previous elevation—from which, they are again displaced, by warmer particles. Thus, the temperatures reversing the situations, and the situations reversing the temperatures, a circulation is kept up, tending to restore the equilibrium.

Precisely similar would be the case with our atmosphere, were it not an elastic fluid, and dependent for its density, on

* Read May 14th, 1822, before the Academy of Natural Sciences of Philadelphia, in whose Journal it was first published.

pressure, as well as on heat. Its temperature would be much more uniform than at present—and all its variations would be gradual. An interchange of position would incessantly take place, between the colder air of the upper regions, and the warmer, and of course lighter, air, near the earth's surface, where there is the most copious evolution of solar heat. Currents would incessantly set from the poles to the equator below, and from the equator to the poles above. Such currents would constitute our only winds, unless where mountains might produce some deviations. Violent gales, squalls, or tornadoes, would never ensue. Gentler movements would anticipate them. But the actual character of the air, with respect to elasticity, is the opposite of that which we have supposed. It is perfectly elastic. Its density is dependent on pressure, as well as on heat; and it does not follow, that air which may be heated, in consequence of its proximity to the earth, will give place to colder air from above. The pressure of the atmosphere varying with the elevation, one stratum of air may be as much rarer by the diminution of pressure, consequent to its altitude, as denser by the cold, consequent to its remoteness from the earth—and another may be as much denser by the increased pressure arising from its proximity to the earth, as rarer, by being warmer. Hence when unequally heated, different strata of the atmosphere do not always disturb each other. Yet after a time, the rarefaction in the lower stratum, by greater heat, may so far exceed that in an upper stratum, attendant on an inferior degree of pressure, that this stratum may preponderate, and begin to descend. Whenever such a movement commences, it must proceed with increasing velocity; for the pressure on the upper stratum, and of course its density and weight, increases as it falls; while the density and weight of the lower stratum, must lessen as it rises. Hence the change is, at times, so much accelerated, as to assume the characteristics of a tornado, squall, or hurricane. In like manner may we suppose, the predominant gales of our climate to originate. Dr. Franklin, long ago, noticed, that north-eastern gales are

felt in the south-westernmost portions of the continent first—the time of their commencement being found later, as the place of observation is more to the windward.

The Gulf of Mexico is an immense body of water—warm, in the first place, by its latitude—in the second place, by its being a receptacle of the current produced by the trade winds, which blow in such a direction, as to propel the warm water of the torrid zone into it, causing it to overflow and produce the celebrated Gulf Stream, by the ejection, to the north-east, of the excess received from the south-east. This stream runs away to the northward and eastward of the United States, producing an unnatural warmth in the ocean, as well as an impetus, which, according to Humboldt, is not expended, until the current reaches the shores of Africa, and even mixes with the parent flood under the equator. The heat of the Gulf Stream, enables mariners to ascertain, by the thermometer, when they have entered it: and in winter, this heat, by increasing the solvent power of the adjoining air, loads it with moisture—which, on a subsequent reduction of temperature, is precipitated in those well known fogs, with which the north-eastern portion of our continent, and the neighbouring seas and islands, especially Newfoundland and its banks, are so much infested. An accumulation of warm water in the Gulf of Mexico, adequate thus to influence the ocean at the distance of two thousand miles, may be expected, in its vicinity, to have effects proportionally powerful. The air immediately over the Gulf, must be heated, and surcharged with aqueous particles. Thus it will become comparatively light; first, because it is comparatively warm—and in the next place, because aqueous vapour, being much lighter than the atmospheric air, renders it more buoyant by its admixture.

Yet the density, arising from inferiority of situation in the stratum of air immediately over the Gulf, compared with that of the volumes of this fluid lying upon the mountainous country beyond it, may, to a certain extent, more than compensate for the influence of the heat and moisture derived

from the Gulf : but violent winds must arise, as soon as these causes predominate over atmospheric pressure, sufficiently to render the cold air of the mountains heavier.

When, instead of the air covering a small portion of the mountainous or table land in Spanish America, that of the whole north-eastern portion of the North American continent, is excited into motion, the effects cannot but be equally powerful, and much more permanent. The air of the adjoining country, first precipitates itself upon the surface of the Gulf, and afterwards, that, from regions more distant. Thus a current from the north-eastward, is produced below. In the interim, the air displaced by this current, rises, and being confined by the table land of Spanish America, and in part, possibly, by the trade winds, from passing off in any southernly course, it is, of necessity, forced to proceed over our part of the continent, forming a south-western current above us. At the same time, its capacity for heat being enlarged, by the rarefaction arising from its increased altitude, much of its moisture will be precipitated—and the lower stratum of the south-western current, mixing with the upper stratum of the cold north-eastern current below—there must be a prodigious condensation of aqueous vapour.

The reason is obvious, why this change is productive only of north-eastern gales—and that we have not northern gales, accompanied by the same phenomena. The course of our mountains, is from the north-east to the south-west. Thus no channel is afforded for the air proceeding to the Gulf, in any other course, than that north-eastern route which it actually pursues.

That the table lands of Mexico, are competent to prevent the escape, over them, of the moist warm air, displaced from the surface of the Gulf, must be evident, from the peculiar dryness of their climate—and the testimony of Humboldt.—According to this celebrated traveller, the clouds formed over the Gulf, never rise to a greater height than four thousand nine hundred feet—while the table land, for many hundred leagues, lies between the elevation of seven and nine

thousand feet. Consistently with the chemical laws, which have been experimentally ascertained to operate throughout nature, air, which has been in contact with water, can neither be cooled nor rarefied, without being rendered cloudy by the precipitation of aqueous particles. It follows, that the air displaced suddenly from the surface of the Gulf of Mexico, by the influx of cold air from the north-east, never rises higher than the elevation mentioned by Humboldt, as infested by clouds. Of course, it never crosses the table land, which, at the lowest, is 2000 feet higher.

Our north-western winds are produced, no doubt, by the accumulation of warm moist air upon the surface of the ocean, as those from the north-east are, by its accumulation on the Gulf of Mexico. But in the case of the Atlantic, there are no mountains to roll back, upon our hemisphere, the air displaced by the gales which proceed from it, and to impede the impulse, thus received, from reaching the Eastern Continent. Our own mountains may procrastinate the flood, and consequently render it more lasting and violent, when it can no longer be restrained. The direction of the wind, is naturally at right angles to the boundary of the aquatic region producing it, and to the mountainous barrier which delays the crisis.

The course of the North American coast is, like that of its mountains, from north-east to south-west—and the gales, in question, are always nearly north-west, or at right angles, to the mountains, and the coast. The dryness of our north-west wind, may be ascribed not only to its coming from the frozen zone, where cold deprives the air of moisture, but likewise to the circumstance above suggested, that the air of the ocean is not, like that of the Gulf, forced back over our heads, to deluge us with rain.

Other important applications may be made of our chemical knowledge. Thus, in the immense capacity of water for heat, especially when vaporized, we see a great magazine of nature, provided for mitigating the severity of the winter.—To cool this fluid, a much greater quantity of matter must

sustain a proportionable increase of its sensible heat.—
 Aqueous vapour is incessantly a vehicle for conveying the caloric of warmer climates, to colder ones. Mistaking the effect for the cause, snow is considered as producing cold, by the ignorant; but it has been proved, that as much heat is given out, during the condensation of aqueous vapour, as would raise twice its weight of glass to a red heat. Water, in condensing from the aëriform state, will raise ten times its weight one hundred degrees. The quantum of caloric, which can raise ten parts one hundred degrees, would raise one part one thousand degrees nearly, (or to a red heat visible in the day)—and this is independent of the caloric of fluidity, which would increase the result.

Further—the quantum of heat which would raise water to 1000, would elevate an equal bulk of glass to 2000. Hence we may infer, that from every snow, there is received twice as much caloric, as would be yielded by an equal depth of red hot powdered glass.

It is thus that the turbulent wave, which at one moment rocks the mariner's sea-boat, on the border of the torrid zone—transformed into a cloud, and borne away towards the arctic, soon after supports the sledge, or the snow-shoe, of an Esquimaux or Greenlander; successively cooling or warming the surrounding media, by absorbing or giving out the material cause of heat.

ESSAY,

ON THE QUESTION,

WHETHER THERE BE TWO ELECTRICAL FLUIDS, ACCORDING
TO DU FAYE—OR ONE, ACCORDING TO FRANKLIN.

By those who allege the existence of two electrical fluids, much stress has been laid on the fact, that light bodies, when negatively electrified, separate from each other no less, than when in the opposite state. The absence and presence of a fluid, cannot, it is said, have the same effect of producing repulsion. To this, it has been answered, that the separation of such light bodies, is not the effect of repulsion, but of an attraction between them and the surrounding medium; which must equally ensue, whether they be electrified minus or plus: since, in either case, that diversity of electrical excitement, between them and the surrounding medium, arises, which is always productive of attraction.

In support of this view of the question, I propose to make a few observations.

In an electroscope with moveable coatings, like the galvanometer of Mr. Pepys,* the divergence of the leaves is faci-

* On the glass of the gold-leaf electrometer, small oblong strips of tin foil are usually pasted, on each side of the gold leaves. As these partial coatings extend to the metallic foot of the instrument, they are always neutral, when the foot is not intentionally insulated. Hence, whether the leaves are excited positively, or negatively, they are attracted, or made to diverge from each other, by means of the coatings. Instead of tin foil, pasted to the glass, Mr. Pepys employed strips of sheet brass, supported by screws. In proportion as these pieces of brass, thus substituted for the usual coatings, were, by means of the screws, approximated to the axis of the instrument, where the gold leaves were hanging, less excitement was found necessary to render them divergent.

itated, in proportion as the coatings are approximated to them. In this case, it must be admitted, that there is an attraction between the coatings and the leaves; for, were repulsion between the leaves the cause of their divergence, the approach of the coatings would not increase it.

It may, however, be supposed, that the repulsion between the similarly excited leaves, being counterbalanced, more or less, in all cases, by the electric tension of the surrounding medium, the coatings may permit the electric fluid to recede through them with greater facility—and thus lessen the electric tension, in the direction in which they are situated.

Were this supposition to avail, in the case of an electrometer with two leaves, it cannot apply in the case of an instrument lately contrived by me, in which, uninfluenced by the idea, that repulsion is the cause of electrometrical indications, I suspend only a single leaf. A brass ball, one-fourth of an inch in diameter, is so situated, that it may be made to touch the leaf, or retire from it to the distance of an inch, by means of a screw which supports it. (See plate I. fig. 1.) This instrument is evidently more simple, than any instrument with two leaves, and is at least as sensitive.*

It will be admitted, I presume, that the contact between the ball and the leaf, must result from attraction, whether the leaf be minus, or plus; and that this would not cease to be true, although a second leaf were, as usual, suspended beside the first.

In a common electrometer, it is usual to have pieces of tin foil pasted on the glass case opposite the gold leaves. If attraction be exercised between the leaves and coatings, when moveable, it must also be exercised by the fixed coatings, thus pasted on the glass. It is therefore established, that, when coatings, whether moveable or fixed, are employed, the divergence is not caused by repulsion. It cannot, then, be

* By means of an instrument with a single leaf, since constructed, I am enabled to detect the electricity produced, by one contact, between a copper and a zinc disk, each six inches in diameter.

reasonable to ascribe it to repulsion, though no coatings should be present, as when the leaves are suspended where nothing can attract them, unless the surrounding air; especially, as the air may be shown competent to perform the same office as the coatings, though not so well, on account of its presenting less matter within the same space. The lightness and mobility of the air, is no obstacle to this conclusion. When equally acted on in all directions, as it must be in the case in point, air resists like an arch, or an elastic solid. The electric attraction may have a tendency to condense it about the sphere of excitement, but cannot move one portion more than another. —This opinion of the agency of the air, is supported by the fact, that, in proportion as an exhausted receiver is larger, so will the difficulty of producing a divergency in the electrometrical leaves, situated within it, be increased. It would be difficult to procure a receiver so large, that gold leaves might not be made to diverge electrically in it, when exhausted; — but leaves of light paper, which will easily be made divergent, in pleno, or in vacuo, in a small vessel, will cease to be affected by a like influence, if suspended in an exhausted receiver sufficiently large. I am aware, that the air prevents the electric fluid from escaping, by its insulating power, and that when it is removed, electrometrical leaves cannot be sustained in a state of excitement, much higher than the rare medium about them. Thus situated, it may be alleged, that repulsion can no more act between them, to produce separation, than it does without them, to keep them together. But this reasoning would apply, equally, whether they be in a large, or in a small receiver; and, of course, does not account for the influence which the size of the receiver has on the divergency.

I will now adduce some additional facts and arguments, in opposition to the doctrine of two fluids.

According to Franklin, positive and negative, as applied to electricity, merely designate relative states of the same fluid. If, of three bodies, the first have more electricity than the second, and less than the third, it will be positive with re-

spect to the second, and negative with respect to the third. According to Du Faye, there is a radical difference between vitreous and resinous electricity—and though separately exercising intense action, they neutralize each other by union. It is universally admitted, that the fluid evolved by the prime conductor of a glass cylinder machine, and that evolved by the cushion, are of different kinds or states. According to the American theory, the first is positive, the last negative. According to the French theory, the first is vitreous, the last resinous.

Let there be two machines, No. 1, and No. 2, so arranged,* that the positive or vitreous conductor of one, may communicate with the negative or resinous conductor of the other.—In this case, the conductors, thus associated, form effectively, but one conducting mass; and one body, with a cushion on one side, and collecting points on the other, might be substituted for both. When this compound apparatus is put into action, it will be found that the intermediate conductor, tested by the resinous conductor of No. 1, is vitreous—but that it is resinous, when tested by the prime or vitreous conductor of No. 2. This result agrees with Franklin's doctrine, as above stated; but how can it be reconciled with the idea, that the electricities are radically different—that the same state of excitement may be confounded with either? It may, indeed, be alleged, that the fluid is never completely vitreous, or resinous, or neutral; that although the proportion of either fluid be great, it may still be increased; that one conductor may be more vitreous than a second, but less so than a third—or more resinous than a second, but less so than a third; and hence, in either case, may give sparks with either. This is, to me, nevertheless, a complicated and unsatisfactory solution of the difficulty.

Pursuant to the Franklinian theory, there can be no really neutral point; though the earth, as a reservoir, infinitely great, compared with any producible by art, furnishes an invariable

* See plate I. fig. 2.

standard of intensity, above and below which, all bodies electrically excited, are said to be minus, or plus.* It is perfectly consistent with this theory, that sparks should pass, as they are often seen to do, from conductors in either state—not only from one to the other, but to bodies nominally neutralized by their communication with the earth. As the difference between the electrical states of the oppositely electrified bodies, must be greater than between either of their states, and that of the great reservoir—the sparks between them will be longer, but, in all other characteristics, will be the same.—This practical result, is irreconcilable with the doctrine of two fluids—according to which, there can be no electricity in the earth, which is not in the state of a neutral compound, formed by these opposite electricities. For it would be an anomaly, to suppose, the reaction between a neutral compound, (*a tertium quid*) and either of its ingredients, to resemble, in intensity, and in its characteristic phenomena, the reaction which arises between the ingredients themselves.—As well might we expect aqueous vapour to explode with hydrogen or oxygen gas, as they do with each other.—Nothing can be more at war with the doctrine of definite proportions, of multiple volumes, and every analogy established by the chemistry of ponderable matter, than that two substances should combine, in every possible proportion, and with precisely the same phenomena; that they should be capable of neutralizing each other, and yet eagerly act as if never neutralized.

An argument in favour of the existence of two fluids, has been founded on the appearance of two burs, when a card is

* In some discussions which took place some years ago, between Mr. Donovan and Mr. De Luc, in Nicholson's Journal, it was erroneously charged against Franklin's doctrine, that he supposed that there was an absolute state of neutrality. The doctrine of one universal fluid, is, to me, obviously irreconcilable with that idea, otherwise than as above explained.—The quantity of electricity in the globe, is as unalterable in any sensible degree, as the quantity of water in the ocean; and it may therefore be assumed to be invariably the same.

pierced by an electric discharge. This phenomenon is as difficult of explanation, agreeably to Du Faye's theory, as Franklin's. If a current of electricity, flowing in one direction, should produce a bur, in piercing a card on the side towards which it flows, two currents should be productive of none—one current being precisely adequate to neutralize the other, according to the premises. The appearance may be explained by either doctrine, as resulting from intense attraction between the paper and the knobs transmitting the discharge.

It has been observed, in favour of the French theory, that, when the hands are made the medium of a feeble discharge, a shock is felt, simultaneously, in the fingers only of each hand; that, as the shock is made stronger, it affects the wrist, the arm, and finally the chest. This is considered as proving the operation of two distinct fluids; for, were the shock the effect of one current, it would be experienced equally, though feebly, throughout the whole of the circuit. Admitting that such a current were necessary to the discharge, agreeably to Franklin's theory, it ought to be felt most in the fingers, where it is most concentrated—as torrents flow with greater violence, in proportion as their channels are narrowed. A current passing from one coating of a Leyden jar to another, is far from being necessary to restore the equilibrium of its surfaces. As soon as a circuit is established between them by the hands, the electricity in the hand which touches the negative surface, flows into it to supply the deficiency; while the hand which touches the positive surface, receives from it a surcharge. It is a case analogous to that of a syphon, in which a fluid, forcibly displaced from its level, is suddenly relieved from restraint. Both columns would move at the same time, and with a velocity greater in any part, in proportion as the diameter of the syphon should be less. The deficit caused in the hand in contact with the negative coating, is supplied by electricity from the arm; and this, again, from the body, where, if the charge be inconsiderable, it is so much diffused as not to be perceived. In like manner, a *slight sur-*

charge received by the hand in contact with the positive coating, is diffused, as it proceeds up the arm to the chest, so as to be too feeble to be felt there.

A piece of tin foil, interposed between paper, has been found not to be perforated by a charge, which had pierced the paper on both sides of it.

If there were but one current, it is alleged that tin foil, situated as above mentioned, would be pierced during its passage from one coating to the other—*a fortiori*, then, it should be pierced, if two currents be necessary, passing each other. Besides, the explanation afforded, in the case of a shock received by the hands, applies to this: owing to its great conducting power, the tin foil diffuses the attraction from each side, so much, as not to be damaged by it.

DESCRIPTION
OF AN
ELECTRICAL PLATE MACHINE,

*The Plate mounted horizontally, and so as to show both
negative and positive Electricity.*

Illustrated by Engravings.

THE power of Electrical Plate Machines, has been generally admitted to be greater, than that of machines with cylinders. —The objection to the former has been, the difficulty of insulating the cushions, so as to display the negative electricity. —Excepting the Plate Machine contrived by Van Marum, I have read of none in which this difficulty has been surmounted. It is still insisted upon, by respectable electricians, as if it had not been sufficiently removed by his contrivance.

I presume, therefore, that a description of a Plate Machine, by which both electricities may be shown, and which, after two years' experience, I prefer on every account, may not be unacceptable to the public.*

My Plate (thirty-four inches in diameter) is supported upon an upright iron bar, about an inch in diameter, covered by a very stout glass cylinder, four inches and a half in diameter, and sixteen inches in height, open only at the base, through which the bar is introduced, so as to form its axis. The summit of the bar is furnished with a block of wood, turned to

* See plate I. fig. 3.

fit the cavity, formed at the apex of the cylinder, and cemented therein. The external apex of the cylinder is cemented into a brass cap, which carries the plate. The glass cylinder is liable to no strain; it is only pressed where it is interposed between the block of wood within, and the brass cap without. The remaining portion of the cylinder bears only its own weight, while it effectually insulates the plate from the iron axis. The brass cap is surmounted by a screw and flange—by means of which, a corresponding nut, and disks of cork, the plate is fastened. A square table serves as a basis for the whole. The iron axis, passing through the cover of the table, is furnished with a wooden wheel of about twenty inches diameter, and terminates below this wheel in a brass step, supported on a cross of wood, which ties the legs of the table diagonally together. The wheel is grooved, and made to revolve by a band, which proceeds from around a vertical wheel, outside of the table. This external wheel has two handles; it may of course be turned by means either of one or both. It is supported on two strips of wood, which, by means of screws, may be protruded, lengthwise, from cases, which confine them from moving in any other direction. By these means, the distance between the wheels may be varied at pleasure, and the tension of the band duly adjusted.

Nearly the same mode of insulation and support, which is used for the plate, is used in the case of the conductors.—These consist, severally, of arched tubes of brass, of about an inch and a quarter in diameter, which pass over the plate from one side of it to the other, so as to be at right angles to, and at a due distance from, each other. They are terminated by brass balls and caps, which last are cemented on glass cylinders, of the same dimensions, nearly, as that which supports the plate. The glass cylinders are suspended upon wooden axes, surmounted by plugs of cork, turned accurately to fit the space which they occupy. The cylinders are kept steady,

below, by bosses of wood, which surround them. In this way, the conductors are effectually insulated, while the principal strain is borne by the wooden axes.

I consider this mode of mounting an Electrical Plate, preferable to any with which I am acquainted. The friction, arising from the band, may render the working of the machine a little harder for one person, with one hand ; but then it affords the advantage, that two persons may be employed for this purpose, or one may use both hands at once. The intervention of the band, secures the plate from being cracked, by a hasty effort to put it into motion, when adhering to the cushions, as it does at times ; and the screws, by means of which the distance of the wheels is increased, obviate the liability of the band to slacken with wear.

ACCOUNT
OF AN
ELECTROMETER WITH A SINGLE LEAF,*

By which the Electricity, excited by the touch of heterogeneous metals, is rendered obvious, after a single contact.†

A SINGLE leaf, suspended from a disk of zinc six inches in diameter, constitutes the top of the instrument. Opposite to this single leaf, is a ball, supported on a wire, which may be made to approach the leaf, or recede from it, by means of a screw. A disk of copper, with a glass handle, accompanies the instrument.‡

The electricity produced by the contact of copper and zinc, is rendered sensible in the following manner. Place the disk of copper, on the disk of zinc, (which forms the canopy of the Electrometer)—take the micrometer screw in one hand, touch the copper disk with the other, and then lift this disk from the zinc. As soon as the separation is effected, the

* Since the account of the Electrometer above described, was published, I have found, that an “Electro-micrometer” was employed, by a Mr. Veau Delaunay, in France, in which, either one, or two leaves were suspended, and their sensibility increased, or diminished, by the approximation, or recession, of pieces of metal, by screws. I believe, however, that my contrivance is original, so far as respects the production of electrical movements, by a single contact of heterogeneous metallic surfaces, and making one of the exciting disks serve as a cap of the instrument.

† This instrument is represented in one of the plates at the end of the book, fig. 7.

‡ For the experiment with this Electrometer, a metallic handle would answer. Its being of glass, enabled me to compare the indication, thus obtained, with that obtained by a Condenser.

gold leaf will strike the ball, usually, if the one be not more than a twentieth of an inch, apart from the other.* Ten contacts of the same disks, of copper and zinc, will be found necessary to produce a sensible divergency in the leaves of the Condensing Electrometer. That the phenomenon arises from the dissimilarity of the metals, is easily shown, by repeating the experiment with a zinc disk, in lieu of a disk of copper. The separation of the homogeneous disks, will not be found to produce any contact, between the leaf and ball.

I believe no mode has been heretofore contrived, by which the electrical excitement, resulting from the contact of heterogeneous metals, may be detected by an Electroscope, without the aid of a condenser.

It is probable, that the sensibility of this instrument, is dependent on that property of electricity, which causes any surcharge of it, which may be created in a conducting surface, to seek an exit at the most projecting termination, or point, connected with the surface. This disposition is no doubt rendered greater, by the proximity of the ball, which increases the capacity of the gold leaf to receive the surcharge, in the same manner, as the uninsulated disk of a condenser, influences the electrical capacity of the insulated disk, in its neighbourhood.

It must not be expected, that the phenomenon above described, can be produced in weather unfavourable to electricity. Under favourable circumstances, I have produced it, by means of a smaller Electrometer, of which the disks are only two and a half inches in diameter.†

The construction, as respects the leaf, and the ball, regulated by the micrometer screw, remaining the same—the cap of a Condensing Electrometer, and its disks, may be substituted for the zinc disk.

* I have observed it to strike at nearly double this distance.

† I think I have seen an effect from a disk only an inch in diameter, or from a zinc disk, having a copper socket to its handle.

A MEMOIR

ON

SOME NEW MODIFICATIONS OF GALVANIC APPARATUS,

WITH

Observations in support of his Theory of Galvanism.

BY ROBERT HARE, M. D.

Professor of Chemistry in the University of Pennsylvania.*

I HAD observed that the ignition, produced by one or two Galvanic pairs, attained its highest intensity, almost as soon as they were covered by the acid employed to excite them, and ceased soon afterwards—although the action of the acid should have increased during the interim. I had also remarked, in using an apparatus of three hundred pairs of small plates, that a platina wire, (number sixteen) placed in the circuit, was fused, in consequence of a construction, which enabled me to plunge them all nearly at the same time. Hence it was conceived, that, in the most improved modes, previously pursued, of operating with extensive Voltaic series, the maximum of deflagrating power, could not have been attained. The plates had been generally arranged in distinct troughs, rarely containing more than twenty pairs. Those of the great apparatus of the Royal Institution, employed by

* First published in the Philadelphia Medical Journal.

Sir Humphry Davy, had only ten pairs in each. Two thousand such pairs were to be placed in the acid, in two hundred such troughs; and the whole of these to be connected, ere the poles could act. Consequently, the effect which arises, immediately after immersion, would be lost in the troughs, first arranged, before it could be produced in the last; and no effort appears to have been made, to take advantage of this transient accumulation of power, either in using that magnificent combination, or any other of which I have read.

In order to observe the consequence of simultaneous immersion, with a series sufficiently numerous to test the correctness of my expectations, eighty concentric coils of copper and zinc, duly connected by arches of metal, were so suspended by a beam and levers, as that they might be made to descend into, or rise out of, the acid in an instant. The zinc sheets were about nine inches by six, the copper fourteen by six—more of this metal being necessary, as in every coil it was made to commence within the zinc, and completely to surround it without. The sheets were coiled, so as not to leave between them an interstice wider than a quarter of an inch. Each coil is in diameter about two inches and a half, so that all may descend freely into eighty glass jars, two inches and three quarters diameter inside, and eight inches high, duly stationed to receive them.*

My apparatus, being thus arranged, two small lead pipes were severally soldered to each pole, and a piece of charcoal, about a quarter of an inch thick, and an inch and a half long, tapering a little at each extremity, had these severally inserted into the hollow ends of the pipes. The jars being furnished with diluted acid, and the coils suddenly lowered into them, no vestige of the charcoal could be seen: it was ignited so intensely, that those portions of the pipes, by which it had been embraced, were destroyed.

In order to avoid a useless and tiresome repetition, I will here state, that the coils were only kept in the acid while the

* See 1st Plate of the Galvanic Deflagrator.

action at the poles was at a maximum, in the experiment just mentioned, and in others which I am about to describe, unless where the decomposition, produced by water, is spoken of, or the sensation excited in the hands. I designate the apparatus with which I performed them, as the Galvanic Deflagrator, on account of its superior power, in proportion to its size, in causing deflagration; and as, in the form last adopted, it differs from the Voltaic pile, in the omission of one of the elements, heretofore deemed necessary to its construction.

Desirous of seeing the effect, of the simultaneous immersion of my series, upon water—the pipes soldered to the poles, were introduced into a vessel containing that fluid. No extraordinary effect was perceived, until they were very near, when happening to touch each other, almost at the same time a vivid flash was observed, and they were found fused and incorporated, at the place of contact. I next soldered, to each pipe, a hollow brass cylinder, the bore being five-tenths of an inch in diameter. These cylinders were made to receive the tapering extremities of a piece of charcoal, about two inches long, so as to complete the circuit. The submersion of the coils, caused the most vivid ignition in the coal.—It was instantaneously, and entirely, on fire. A piece of platina, of about a quarter of an inch diameter, in connexion with one pole, was instantly fused, at the end, on being brought in contact with some mercury, communicating with the other pole. When two cylinders of charcoal, having hemispherical terminations, were fitted into the brass cylinders, duly connected with the apparatus, and brought nearly into contact, a most vivid ignition took place, and continued after they were removed, about a half or three quarters of an inch apart—the interval rivalling the sun in brilliancy. The most intense action seems to arise, from placing a platina wire, of about the eighth of an inch diameter, in connexion with one pole, and bringing it in contact with, and afterwards removing it a small distance apart from, a piece of charcoal (fresh from the fire) affixed to the other pole.

As points are pre-eminently capable of carrying off, without being injured, a current of the electrical fluid, and very ill qualified to conduct caloric—while, by facilitating radiation, charcoal favours the separation of caloric from the electricity which does not radiate. This result seems consistent with my theory, that caloric and electricity are evolved by the pile, as collateral products—but is inconsistent with the doctrine, according to which the caloric is an effect of electricity.* The finest needle is competent to discharge the product of the most powerful machine without detriment, if received gradually as generated by them. Platina points, as small as those which were melted like wax in my experiments, are used as tips to lightning rods without injury, unless in sudden discharges produced under peculiar circumstances.†

* According to the theory here alluded to, the Galvanic fluid owes its properties to caloric and electricity—the former predominating in proportion to the size of the pairs—the latter in proportion to the number: being in both cases excited by a powerful acid. Hence in batteries, which combine both qualifications sufficiently, as in all those intervening between Children's large pairs of two feet eight inches by six feet, and the 2000 four-inch pairs of the Royal Institution, the phenomena indicate the presence of both fluids. In De Luc's column, where the size of the pairs is insignificant, and the energy of interposed agents feeble, we see electricity evolved, without any appreciable quantity of caloric. In the calorimotor, where we have size only, the number being the lowest possible, we are scarcely able to detect the presence of electricity.

When the fluid contains enough electricity, to give a projectile power adequate to pass through a small space in the air, or through charcoal, which impedes or arrests the caloric, and favours its propensity to radiate, this principle is evolved. Hence the intense heat, observed under those circumstances which rarefies the air, so that the length of the jet, from one pole to the other, may be extended after its commencement: hence the portions of the circuit nearest to the intervening charcoal, or heated space, are alone injured; and even non-conducting bodies, as quartz, introduced into it, are fused—and hence a very large wire may be melted by the fluid, received through a small wire imperceptibly affected.

See Silliman's Journal, No. 6, Vol. 1.—Thomson's Annals, Sept. 1810.—Tilloch's Philosophical Magazine, Oct. 1819.

† See Lecture on Electricity, page 23 of this Supplement.

The following experiment, I conceive to be very unfavourable to the idea, that Galvanic ignition arises from a current of electricity, unaccompanied by a current of caloric.

A cylinder of lead, of about a quarter of an inch diameter, and about two inches long, was reduced to the thickness of a common brass pin, for about three quarters of an inch.—When one end was connected with one pole of the apparatus, the other remained suspended by this filament—yet it was instantaneously fused, by contact with the other pole. As all the calorific fluid which acted upon the suspended knob, must have passed through the filament by which it hung, the fusion could not have resulted from a pure electrical current, which would have dispersed the filament, ere a mass, fifty times larger, had been perceptibly affected. But if we suppose a union between caloric and electricity, which renders them inert till circumstances favour a disunion, as on the passage of the compound fluid through charcoal, the air, or a vacuum, the phenomena are not so inexplicable.

In operating with the deflagrator, I have found a brass knob, of about five-tenths of an inch in diameter, to burn on the superficies only—where alone, as I suppose, caloric is separated, so as to act on the mass.

Having found, that four Galvanic surfaces acted well in one recipient,* I was tempted, by means of the eighty coils, to extend that construction. It occurred to me, that attempts of this kind had failed, from using only one copper for each zinc plate. The zinc had always been permitted to react towards the negative, as well as the positive pole. My coils being surrounded by copper, it seemed probable, that, if electro-caloric were, as I had suggested, carried forward by circulation, arising from Galvanic polarity, this might act within the interior of the coils, yet not be exerted between one coil and another. I had, accordingly, a trough constructed with a partition along the middle, so as to receive

* See Plate of the Calorimotor, at the end of the book.

forty coils on one side, and a like number on the other.—This apparatus, when in operation, excited a sensation, scarcely tolerable, in the backs of the hands—and a most intense ignition took place, on bringing a metallic point, connected with one pole of the series, into contact with a piece of charcoal fastened to the other. A cylinder of platina, nearly a quarter of an inch in diameter, and tapering a little at the end, was fused, and burned, so as to sparkle and fall in drops. A ball of brass, of about half an inch diameter, was seen to burn on its surface with a green flame. Tin foil, or tinsel, rolled up into large coils of about three quarters of an inch thick, were rapidly destroyed, as was a wire of platina of No. 16. When platina wires in connexion with the poles, were brought into contact with sulphuric acid, there was an appearance of lively ignition.

Apprehending that the partition in the trough did not sufficiently insulate the poles from each other, as they were but a few inches apart, moisture or moistened wood intervening, I had two troughs made, each to hold forty pairs, and took care that there should be a dry space, about four inches broad, between them. They were first filled with pure river water, there being no saline nor acid matter to influence the plates, unless the very minute quantity which might have remained on them from former immersions. Yet the sensation produced by them, on the backs of my hands, was painful—and a lively scintillation took place, when the poles were approximated. Dutch gold leaf was not sensibly burned, though water was found decomposable by wires properly affixed.—No effect was produced on potash, the heat being inadequate to fuse it.

A mixture of nitre and sulphuric acid, being added to the water in the troughs, charcoal from the fire was afterwards vividly ignited—and when, attached to one pole, a steel wire was interposed between it and the other pole, the most vivid ignition which I ever saw, was induced. I should deem it imprudent to repeat the experiment without glasses, as my eyes, though unusually strong, were affected for forty-eight

hours afterwards. If the intensity of the light did not produce an optical deception, by its distressing influence upon the organs of vision, the charcoal assumed a pasty consistence, as if in a state approaching to fusion. That charcoal should be thus softened, without being destroyed by the oxygen of the atmosphere, may be ascribed to the excessive rarefaction, or, as I suspect, to the protection afforded, by an atmosphere of volatilized carbon. This last mentioned impression arose from observing, that when the experiment was performed in vacuo, there was a lively scintillation, as if the carbon, in an aëriform state, acted as a supporter of combustion on the metal.

A wire of platina (No. 16) was fused into a globule on being connected with the positive pole, and brought into contact with a piece of pure hydrate of potash, situated on a silver tray in connexion with the other pole. The potash became red hot, and was deflagrated rapidly, with a flame having the rosy hue of potassuretted hydrogen.

A steel wire of about one-tenth of an inch in diameter, affixed to the negative pole, was passed up through the axis of an open necked inverted bell glass, filled with water. A platina wire, No. 16, attached to a positive pole, being passed down to the steel wire, both were fused together, and cooling, could not be separated by manual force. Immediately after this incorporation of their extremities, the platina wire became incandescent for a space of some inches above the surface of the water.

A piece of silvered paper about two inches square, was folded up, the metallic surface outward, and fastened into vices affixed to the poles. Into each vice a wire was screwed at the same time. The fluid generated by the apparatus, was not perceptibly conveyed by the silvered paper, as it did not prevent the wires severally attached to the poles, from decomposing water, or producing ignition by contact.

In my Memoir on my theory of Galvanism, I suggested, that the decomposition of water, which Wollaston effected

by mechanical electricity, might not be the effect of divellent attraction, like those excited by the poles of a Voltaic pile, but of a mechanical concussion, as when wires are dispersed by the discharge of an electrical battery. In support of that opinion, I will now observe, that he could not prevent hydrogen and oxygen from being extricated at each wire, instead of hydrogen being given off only at one, and oxygen at the other, as is invariably the case when the Voltaic pile is employed. That learned and ingenious philosopher, in concluding his account of this celebrated experiment, says:—“But in fact the resemblance is not complete, for in every way in which I have tried it, I observed each wire gave out both oxygen and hydrogen gas, instead of their being formed separately as by the electric pile.”

Is it not reasonable to suppose, that an electrical shock may dissipate any body into its elementary atoms, whether simple or compound, so that no two particles would be left together, which can be separated by physical means?

Looking over Singer's *Electricity*, a recent and most able modern publication, I find, that in the explosion of brass wire by an electrical battery, the copper and zinc actually separated.

He says, page 186, “Brass wire is sometimes *decomposed* by the charge: the copper and zinc, of which it is formed, being separated from each other, and appearing in their distinct metallic colours.” On the next page in the same work, I find, that the oxides of mercury, and tin, are reduced by electrical discharges. “Introduce,” says the author, “some oxide of tin, into a glass tube, so that when the tube is laid horizontal, the oxide may cover about half an inch of its lower internal surface. Place the tube on the table of the universal discharger, and introduce the pointed wires into its opposite ends, that the portion of oxide may lay between them. Pass several strong charges in succession through the tube, replacing the oxide in its situation, should it be *dispersed*. If the charges are sufficiently powerful, a part of the tube will

soon be stained with metallic tin, which has been revived by the action of transmitted electricity.”

It cannot be alleged, that, in the decompositions described in this quotation, those divellent polar attractions are exercised, which characterise wires proceeding from the poles of a Voltaic apparatus. The particles were *dispersed from*, instead of being *attracted to*, the wires, by which the influence was conveyed among them. This being undeniable, it can hardly be advanced that, while we have one mode of explaining the separation of the elements of brass, by an electrical discharge—another is to be resorted to, in explaining the separation of the elements of water, by the same agent: that one rationale is to be adopted, in order to account for the liberation of oxygen from tin—and another, when it is liberated by like means from hydrogen.

In the experiment, in which copper was precipitated, by the same philosopher, at the negative pole, we are not informed whether the oxygen and acid, in union with it, were attracted to the other—and the changes produced in litmus, are mentioned as successive, not as simultaneous. The violet and red rays of the spectrum, have an opposite chemical influence, in some respects analogous to that of the Voltaic poles—but it does not follow, that Galvanism and light, are produced by the same cause. Besides admitting, that the feeble results, obtained by Wollaston and Van Marum, with mechanical electricity, are perfectly analogous to those ordinarily obtained by the Galvanic fluid,—before they can furnish an objection to my hypothesis, it ought to be shown, that the union between caloric and electricity, which I suppose productive of Galvanic phenomena, cannot be produced by that very process. If they combine to form the Galvanic fluid, when extricated by ordinary Galvanic action, they must have an affinity for each other. When electricity enters the pores of a metal, it may unite with its caloric. In Wollaston’s experiments, being constrained to enter the metal, it may combine with enough of its caloric to produce, when emitted,

results slightly approaching to those of a fluid, in which caloric exists in greater proportion.

But, once more I demand, why, if mechanical electricity be too intense to produce Galvanic phenomena, should it be rendered more capable of producing them, by being still more concentrated?

If the one be generated more copiously, the other more intensely,—the first will move slowly in a large stream, the last rapidly in a small stream. Yet, by narrowing the channel of the more rapid current, Wollaston is supposed to render it more like the other—that is, produces a resemblance, by magnifying the feature in which the dissimilitude exists.

It has been imagined, that the beneficial effect of his contrivance, arises from the production of a continued stream, instead of a succession of sparks; but if a continued stream were the only desideratum, a point, placed near the conductor of a powerful machine, would afford this requisite—as the whole product may, in such cases, be conveyed by a sewing needle, in a stream perfectly continuous.

As yet, no adequate reasons have been given why, in operating with the pile, it is not necessary, as in the processes of Van Marum and Wollaston, to enclose the wires in glass or sealing wax, in order to make the electricity emanate from a point within a conducting fluid. The absence of this necessity, is accounted for, according to my hypothesis, by the indisposition which the electric fluid has to quit the caloric in union with it, and the almost absolute incapacity which caloric has to pass through fluids, unless by circulation. I conceive, that, in Galvanic combinations, electro-caloric may circulate through the fluid, from the positive to the negative surface—and through the metal, from the negative to the positive. In the one case, caloric subdues the disposition which electricity has to diffuse itself through fluids, and carries it into circulation. In the other, as metals are excellent conductors of caloric, the prodigious power which electricity has to pervade them, agreeably to any attractions which it

may exercise, operates almost without restraint. This is fully exemplified in the Galvanic Deflagrator, where eighty pairs are suspended, without insulation, in two recipients, forty successively in each, and yet decompose potash with the utmost rapidity, and produce an almost intolerable sensation,* when excited only by fresh river water.

I have already observed, that the reason why Galvanic apparatus, composed of pairs consisting each of one copper and one zinc plate, has not acted well without insulation,† was, because in such apparatus, electro-caloric could retrocede in the negative, as well as advance in the positive direction.

Pursuant to the doctrine, which supposes the same quantity of electricity, varying in intensity in the ratio of the number of pairs to the quantity of surface, to be the sole agent in Galvanic ignition—the electrical fluid, as evolved by Sir H. Davy's great pile, must have been nearly two thousand times more intense, than as evolved by a single pair; yet it gave sparks, at no greater distance than the thirtieth or fortieth of an inch. The intensity of the fluid, must be at least as much greater in one instance, than in another, as the sparks produced by it are longer. A fine electrical plate machine, of thirty-two inches diameter, will give sparks at ten inches.—Of course the intensity of the fluid which it emits, must be three hundred times greater, than that emitted by two thousand pairs. The intensity produced by a single pair, must be two thousand times less, than that produced by the great pile—and of course six hundred thousand times less, than that produced by a good electrical plate of thirty-two inches. Yet a single pair, of about a square foot in area, will certainly deflagrate more wire, than a like extent of coated surface, charged by such a plate.

* I do not say shock, as it is more like the permanent impression of hot pointed wire, especially when an acid is used.

† That is, with the same mass of conducting fluid, in contact with all the surfaces, instead of being divided into different portions, each restricted in its action to one copper and one zinc plate.

According to Singer, it requires about one hundred and sixty square inches of coated glass, to destroy watch pendulum wire:—a larger wire may be burned off, by a Galvanic battery of a foot square. But, agreeably to the hypothesis in dispute, it compensates, by quantity, for the want of intensity. Hence the quantity of fluid in the pair, is six hundred thousand times greater, while its intensity is six hundred thousand times less—and *vice versa* of the coated surface. Is not this absurd? What does intensity mean, as applied to a fluid? Is it not expressed, by the ratio of quantity to space? If there be twice as much electricity within one cubic inch, as within another, is there not twice the intensity? But, the one acts suddenly, it may be said—the other slowly. But whence this difference? They may both have exactly the same surface to exist in. The same zinc and copper plates may be used for coatings first, and for a Galvanic pair afterwards. Let it be said, as it may in truth, that the charge is, in the one case, attached to the glass superficies—in the other, exists in the pores of the metal. But, why does it avoid these pores in one case, and reside in them in the other? What else resides in the pores of the metal, which may be forced out by percussion? Is it not caloric? Possibly, unless under constraint, or circumstances favourable to a union between this principle and electricity, the latter cannot enter the metallic pores, beyond a certain degree of saturation; and hence an electrical charge does not reside in the metallic coatings of a Leyden phial, though it fuses the wire which forms a circuit between them.

It is admitted, that the action of the Galvanic fluid is upon, or between, atoms—while mechanical electricity, when uncoerced, acts only upon masses. This difference has not been explained, unless by my hypothesis, in which caloric, of which the influence is only exerted between atoms, is supposed to be a principal agent in Galvanism. Nor has any other reason been given, that water, which dissipates pure electricity, should cause the Galvanic fluid to accumulate.—

From the prodigious effect which moist air, or a moist surface, has, in paralyzing the most efficient electrical machines, I am led to suppose, that the conducting power of moisture, so situated, is greater than that of water under its surface. The power of this fluid, to conduct mechanical electricity, is unfairly contrasted with that of a metal, when the former is enclosed in a glass tube.

According to Singer, the electrical accumulation is as great when water is used, as when more powerful menstrua are employed; but the power of ignition is wanting, until these are resorted to. De Luc showed, by his ingenious dissections of the pile, that electricity might be produced *without*, or *with*, chemical power. The rationale of these differences, never has been given, unless by my theory, which supposes caloric to be present in the one case, but not in the other.—The electric column was the fruit of De Luc's sagacious inquiries, and afforded a beautiful and incontrovertible support to the objections which he made to the idea, that the Galvanic fluid is pure electricity, when extricated by the Voltaic pile in its usual form. It showed, that a pile, really producing pure electricity, is devoid of the chemical power of Galvanism.

We are informed by Sir H. Davy, that, when charcoal points in connexion with the poles of the magnificent apparatus, with which he operated, were first brought nearly into contact, and then withdrawn four inches apart, there was a heated arch formed between them, in which such non-conducting substances as quartz, were fused. I believe it impossible to fuse electrics by mechanical electricity. If opposing its passage, they may be broken—and if conductors near them be ignited, they may be acted on by those ignited conductors, as if otherwise heated; but I will venture to predict, that the slightest glass fibre will not enter into fusion, by being placed in a current from the largest machine or electrical battery.

I am induced to believe, that we must consider light, as well as heat, an ingredient in the Galvanic fluid ; and think it possible, that, being necessary to vitality in animals, as well as vegetables, the electric fluid may be the vehicle of its distribution.

Since writing the above, I have endeavoured, in every mode which I could devise, to ignite charcoal by electricity. Exposed to the discharge of a powerful battery, in pieces tapering to a point, in a glass tube, in thin strips, and in powder, by means of the glass usually employed for inflaming ether, it was either uninfluenced, or merely dispersed, without the smallest symptom of ignition, or even of increased warmth. Yet fulminating mercury was flashed by the discharge, under the same circumstances as those in which the powdered charcoal had been subjected to it.—Pointed wires were covered with spermaceti, and exposed to a current from a fine plate machine of thirty-two inches in diameter—yet no sign of fusion appeared. Nor was a differential thermometer filled with ether, according to Dr. Howard's plan, affected sensibly ; though the warmth of a finger applied to the bulb, caused the fluid in the stem to move nearly a foot.

I mentioned, (page 33) that when a knob of lead, suspended by a filament to one of the poles of my deflagrator, was made to touch the other pole of the same instrument, the knob was fused, the filament uninjured. I find the reverse is the case, when a knob, suspended by a filament, is made the medium of discharging an electrical battery. The filament is destroyed, the knob remains unchanged. It must be evident, therefore, that Galvanic and electrical ignition, are extremely discordant in their characteristics.

It is also mentioned, (page 35) that a piece of silvered paper two inches square, proved inadequate to discharge my Galvanic apparatus of coils—yet, at a distance seventy times greater, a strip of the same paper, one-third of an inch wide, and twenty inches long, caused an instantaneous discharge of the electrical battery.

LETTER

*From ROBERT HARE, M. D. Professor of Chemistry in the University of Pennsylvania, to B. SILLIMAN, Professor of Chemistry in Yale College, respecting some improved forms of the Galvanic Deflagrator—and on the superiority of its deflagrating power.**

AFTER I had discovered that the deflagrating power, of a series of Galvanic pairs, was surprisingly increased, by their simultaneous exposure, after due repose, to the acid—various modes suggested themselves, of accomplishing this object.—In the apparatus which I sent you, the coils, being all suspended to two beams, could be lowered into troughs, containing the acid.† In another apparatus, of which I afterwards gave you an account, with an engraving for your Journal, the troughs containing the acid, were made to rise, so that all the plates might be immersed at once.‡

A better mode has since occurred to me. Two troughs are joined lengthwise, edge to edge, so that when the sides of the one are vertical, those of the other must be horizontal. Hence, by a partial revolution of the two troughs, thus united, upon pivots which support them at the ends, any fluid which may be in one trough, must flow into the other—and, reversing the motion, must flow back again. The Galvanic series being placed in one of the troughs, the acid in the

* Published first in Silliman's Journal.

† See 1st Plate of the Deflagrator. ‡ See 2d Plate of the Deflagrator.

other, by a movement such as above described, the plates may all be instantaneously subjected to the acid, or relieved from it. The pivots are made of iron, coated with brass or copper, as less liable to oxidizement. A metallic communication is made between the coating of the pivots, and the Galvanic series within. In order to produce a connexion between one recipient of this description, and another, it is only necessary to allow a pivot of each trough to revolve on pieces of sheet copper, severally soldered to the different ends of a rod of metal. To connect with the termination of the series, the leaden rods, (to which are soldered the vices, or spring forceps, for holding the substances to be exposed to the deflagrating power,) one end, of each of the lead rods, is soldered to a piece of sheet copper. The pieces of copper, thus soldered to the lead rods, are then to be duly placed under the pivots, which are of course to be connected with the terminations of the series. The last mentioned connexion is conveniently made by means of straps of copper, severally soldered to the pivots, and the poles of the series, and screwed together by a hand-vice.

Fig. 1, plate 3,* represents an apparatus, consisting of two troughs, each ten feet long, constructed in the manner which I have described. Each trough is designed to contain one hundred and fifty Galvanic pairs. The Galvanic series in the upper trough, is situated as when not subjected to the acid. In the representation of the lower trough, the Galvanic series is omitted, in order that the interior may be better understood. The series belonging to this trough, may be observed below it, in three boxes, each containing fifty pairs, fig. 2. In placing these boxes in the trough, some space is left, between them and that side of the trough on which the acid enters, so that instead of flowing over them, it may run down outside, and rise up within them.

The pairs of the series consist of copper cases, about seven inches long, by three inches wide, and half an inch thick—

* See 3d Plate of the Deflagrator.

each containing a plate of zinc, equidistant from its sides, and prevented from touching it by grooved strips of wood.— Each plate of zinc is soldered to the next case of copper, on one side. This may be understood from the diagram, fig. 3. It must be observed, that the copper cases are open only at the bottom and top. They are separated from each other by very thin veneers of wood.

Fig. 4, represents a smaller trough, differing from the others only in length. This I made, with a view to some experiments on the comparative power of the Galvanic pairs of the form of copper cases, with zinc plates, above described, and those made on Cruickshank's plan, or of the form used by Sir H. Davy, in the porcelain troughs.

Fig. 5, represents a box, containing one hundred Cruickshank plates, (each consisting of a plate of zinc, and a plate of copper, soldered face to face) slid into grooves, at a quarter of an inch distance from each other; all the copper surfaces being in one direction, and all the zinc surfaces in the other. In this case the zinc plates are exposed only on one side.— The sum of the surfaces on which the acid can act, is therefore the same as in a deflagrator of fifty pairs, in which each zinc plate is assailable on both sides. It ought to be understood, that the box containing the one hundred Cruickshank plates, is open at bottom, and is of such dimensions as to occupy the place of a box, containing fifty pairs of the deflagrator, receiving the acid in its interstices from below, in the same manner, by a partial revolution of the trough, fig. 4.

Fig. 6, represents a box, containing two hundred Cruickshank plates. This differs from the common Cruickshank trough, only, in having the interstices as narrow as those between the copper and zinc surfaces of the deflagrator pairs, represented by fig. 2; and in the mode in which the acid is thrown off, or on, the whole series, which does not differ, materially, from that described in the instance of fig. 1.

On contrasting the series of fifty, (fig. 4) with Cruickshank's plates in the box, (fig. 5) the deflagrating power of the latter was found comparatively feeble—and even when

compared with the Cruickshank trough, (fig. 6) in igniting metals, or carbon, the fifty pairs (fig. 4) were found greatly superior. The shock from the Cruickshank trough was more severe. You must recollect, that in former experiments, I found that Galvanic plates, with their edges exposed as they are in the porcelain troughs, used by Sir H. Davy, were almost inefficient, when used without insulation, as are the pairs of the deflagrator. This demonstrates, that an unaccountable difference is producible, in Galvanic apparatus, by changes of form or position.

Being accustomed to associate the idea of the zinc pole, in a Voltaic series, with the end terminated by zinc, and the copper pole, with the end terminated by copper, I was surprised to find that, in decomposing water, the oxygen was attracted by the wire connected with the copper end of the deflagrator, while the hydrogen went to the wire connected with the zinc end. Subsequently, however, it occurred to me, that, in the deflagrator, the zinc pole is terminated by copper, the copper pole by zinc—and hence the apparent anomaly, that oxygen appears to be attracted by copper, and hydrogen to be attracted by zinc.

The projection from the carbon, exposed between the poles, takes place at the negative pole of the pile, and not at the positive pole, as you have alleged; and thus your observation, that the current of igneous matter, is from the copper to the zinc, may be reconciled with the Franklinian theory.

The observations, which are the subject of this communication, combined with those which you have made, of the incapacity of the deflagrator, and Voltaic series in the usual form, to act, when in combination with each other—must justify us, in considering the former, as a Galvanic instrument, having great and peculiar powers.

Since the above was written, I have tried my series of three hundred pairs. The projectile power, and the shock, were proportionally great, but the deflagrating power was not increased in proportion. The light was so intense, that, falling on some adjacent buildings, it had the appearance of sunshine.

Having had another series of three hundred pairs made for Dr. Macneven of New York, on trying it, I connected it with mine, both collaterally, and consecutively, so as to make in the one case a series of six hundred, in the other a series, of half that number, but equal in extent of surface.—The shock of the two, consecutively, was apparently doubly as severe, as the shock produced by one; but the other phenomena seemed to me nearly equally brilliant, in either way.

The white globules which you noticed, were formed copiously on the ignited plumbago, especially in vacuo. I have not had leisure, to test them, being arduously occupied, in my course of Lectures, and in some efforts to improve the means of experimental illustration.



Since this letter was published, I find, that my friend Dr. De Butts of Baltimore, has, in one apparatus, availed himself of that alternation of surfaces, that omission of insulation, which I had first used in one of my Calorimotors;* in another more numerous series of smaller plates, he employs the principle of simultaneous immersion, originally used, with respect to an extensive series, in my Deflagrator. His plates differ from mine, in being semicircular—and there are more pairs in the series, and fewer large plates in each pair, than in the Calorimotor.† In his apparatus, the plates, by a quarter revolution, enter the acid: in mine, a similar movement in the trough, throws the acid on the plates.

I often contemplated the mode which he has adopted, as it seemed sufficiently convenient; but, for several reasons, preferred the methods which I have employed. Cutting the sheets semicircularly, is very wasteful of the metal—and I have never seen sheet zinc, for sale, in the circular form, nor copper either, unless for bottoms, which are too heavy and expensive. Plates of cast zinc cannot be used advantageously,

* Of which a Plate is given, at the end of the book.

† I consider two heterogeneous metallic surfaces, as for instance, one surface of copper and one surface of zinc, when associated by metallic contact, or a metallic strap passing from one to the other, as a Galvanic Pair—whether the surfaces be in one sheet of zinc, and one sheet of copper, or, consist of several sheets of zinc, and several of copper; one metallic communication uniting all the zinc sheets—and another, uniting all the copper sheets.

as they are too heavy for large apparatus, and are soon made rough by corrosion, which diminishes their energy; whereas the rolled zinc may be eaten down as thin as paper, and still be efficient. The rationale of this difference, is to be found in the crystalline texture of the cast zinc, which rolling destroys. That solution, to a certain extent, tends to expose the angles of crystals, has been fully shown, in the decrystallization of alum, first observed, I believe, by Mr. Daniels. I have recently seen it strikingly exemplified.

Experience demonstrates the importance, of encasing each zinc plate, especially if uninsulated, in the copper of the pair, which succeeds it, in the series. In the form adopted by Dr. De Butts, the copper cases cannot be made, without a much greater expense in the workmanship. Those which I have used, are formed by machinery, so that they are very handsomely finished, with great rapidity, and all of one size, and shape.

In consequence of this contrivance, a workman has undertaken to furnish Deflagrators, at forty-five or fifty cents for every Galvanic pair (seven inches by three) contained in them. This is much lower than the Paris prices, for apparatus far less powerful.

I cannot discover the motive of Dr. De Butts, for having the legs of his apparatus of glass—while, from the construction of his troughs within, he evidently sanctions my plan, of omitting insulation. Had I seen the glass legs, without being aware of the internal construction of his battery, I should have expected to find the inside partitioned by glass.

Dr. De Butts speaks of the coils of Col. Offerhaus and Mr. Pepys, as if that form of the Galvanic battery had originated with them; whereas this was one of the forms, first contemplated by me—it was afterwards actually made by Dr. Patterson and Mr. Lukens, and in a much larger form by Mr. Peale and Mr. Wetherill,* at least a year, I believe two years, before it was resorted to either by Pepys, or Offerhaus.

* See Memoir on a New Theory of Galvanism, Silliman's Journal, Vol. I. page 118—also Memoir on the Deflagrator, page 41.